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XXXI. *Extract of a Letter from Mr. Mallet, of Geneva, to Dr. Bevis, F.R.S.*

Read June 21, 1770. **I** HAD the pleasure of writing you a few lines in the month of August, last year, when I sent you my observations relative to the transit of Venus, which the Petersburg Academy has printed without my knowledge, whilst I was yet in Lapland. I left Russia soon afterwards, and have been five or six months in my own country. Part of this time I have employed in reducing and computing my observations made in the North, to get what useful results I could from them, which I have just now sent to Petersburg, to be printed in the Commentaries of the Academy. As it may be some time before that volume will be published, I thought, Sir, you might be willing to be informed of some of the principal consequences resulting from my observations.

1°. To determine the latitude of Ponoï, where I observed the transit, a great number of meridian altitudes of the stars and Sun, taken with a quadrant of 2 feet radius, made at London by Mr. Siffon, gave the elevation of the pole $67^{\circ} 04' 30''$. I was not able to make any other observation but that of the Sun's eclipse, on the 4th of June, for determining the longitude. I observed with a 12 feet achromatic

A a a 2

matic telescope of Dollond, the end at $0^h 07' 55''$ apparent time. The celebrated M. L. Euler, who has computed several observations of this eclipse made in different places, finds, by my observation, the difference of meridians between Paris and Ponoï $2^h 25' 33''$, that is, $38^\circ 51'$ East of Paris.

2°. On the observation of the transit of Venus.

I observed, with the same telescope, the interior contact at the entry at $10^h 15' 4''$ apparent time. I have computed very scrupulously the effect of parallax on the moment of this contact; I made use of the same elements that M. de la Lande gives in his *Memoire*, printed in 1764, excepting that I assume the nearest distance of the centres of Venus and the Sun, seen from the centre of the Earth $10' 27''$; which quantity I deduced from the whole duration, observed at Hudson's Bay, by Mess. Dymond and Wales, as given in the news-papers. I find the effect of parallax $7' 03''$ of time, whereby I must have seen the contact sooner than at the earth's centre. The computation of my observation gives moreover the moment of the conjunction at $12^h 46' 21\frac{1}{2}''$ apparent time at Ponoï, and the geocentric latitude of Venus for that moment $10' 33'',9$.

If the nearest distance of the centres be taken $5''$ less, I find the effect of parallax $7' 11''$ of time, that is, $8''$ greater, the latitude becomes $5''$ less, and the moment of the conjunction $1' 28''$ later.

3°. I have made a great number of observations for determining the force of gravity and the length of the simple pendulum swinging seconds. I used an invariable pendulum which M. de la Condamine got constructed at Quito, when the French academicians
went

went thither to measure a degree of the meridian, which he was pleased to send me to Petersburg; this pendulum, which is no other than a simple steel rod fixed to a lentille, made at Paris 98740 oscillations in 24 hours of mean time, and at Paris 98891 in the same time. I made experiments with this same pendulum at Petersburg, before my departure for Lapland, and have repeated them since my return thither. They give the number of oscillations in 24 hours of mean time 98941, having been careful to preserve constantly the same temperature, and to cause the pendulum to swing very small arcs. At Ponoï, I found the number of oscillations 98946. Hence it follows, that the simple pendulum, which beats seconds at Petersburg, will be 441,02 lines (Paris measure), that is, $\frac{4.5}{1000}$ lin. longer than the pendulum which beats seconds at Paris; and the pendulum at Ponoï will be 441,22 lin. that is $\frac{6.5}{1000}$ lin. longer than that of Paris.

The excess of the Paris pendulum above that at the equator has been determined by the academicians 1,50 lin.; and admitting Sir Isaac Newton's principle, and Huyhens', that the increase of gravity, in approaching the pole, follows the ratio of the square of the sine of latitude, we should find 1,98 lin. for the excess of the Petersburg pendulum above that at the equator, instead of 1,95, which I find by my experiments; the same calculus would give 2,24 lin. for the excess of the Ponoï pendulum, instead of 2,15 lin. which results from my experiments. Hence it would follow that the increments of gravity follow a ratio somewhat greater than that of the squares of the sines of latitude; and this result is confirmed

confirmed by the experiments made at Pello in Lapland, by the French academicians.

4°. I observed several times very exactly at Ponoï, the declination of the magnetic needle $1^{\circ} 10'$ East.

5°. Exact observations of the inclination of the needle made in different places of our globe, combined with those made long ago on the declination, would be very interesting and proper for the advancement of our knowledge, as to the theory of magnetism, which hitherto is but little understood. It is the difficulty of making such observations, and obtaining accurate results, which has discouraged physicians and travellers; but it is surprizing that so little has been done in this matter, since Dr. Daniel Bernoulli furnished us with new ideas for constructing a machine fit for determining the true magnetic inclination, in a Memoire, which gained the prize proposed for this subject by the Paris Academy, in 1743. He got an inclinatory needle constructed at Basle, upon new principles, and the experiments he made assured him of success; he found the inclination at Basle $71\frac{1}{4}$ degrees.

Mr. Euler, the son, made use of the same compass at Berlin, but by employing a method entirely different from that of Dr. Bernoulli. He gives the particulars thereof in the Memoirs of the Academy of Berlin, 1755. After a great number of observations, he found the inclination to be then at Berlin between $72\frac{1}{2}$ and 73 degrees. At Petersburg I got constructed a like machine, and used it for determining the inclination at Petersburg, Kola, and Ponoï; I employed both the methods of Mess. Bernoulli and Euler, and found a wonderful agreement in the results

sults drawn from a great number of experiments. Two needles made by different artists, one at Basle, the other at Petersburg, consequently susceptible of a different magnetic force, produced but very minute differences, inevitable in so delicate experiments; the several particulars are recited in the papers I have sent to the Petersburg Academy, from whence it may be concluded that it is possible to determine with this instrument the true inclination of the magnetic needle, without being any way liable to an error of half a degree in the result, which in my experiments is as follows.

At Petersburg lat. 59° 55'	longitude 48°	the inclination was in 1769	73 $\frac{2}{3}$
At Ponoï	67 04	58° 51'	76 $\frac{1}{2}$
At Kola	68 54	49 45	77 $\frac{3}{4}$

6°. I have moreover subjoined the several particulars of my meteorological observations, during my five months stay in Lapland. Let it suffice to give you the mean height of the barometer, which I found 27 in. 6,2 lin. (Paris) in March; 27 in. 5,5 lin. in April; 27 in. 7,6 lin. in May; and 27 in. 5,8 lin. in June: the mean height for four months being 27 in. 6 $\frac{1}{3}$ lin. I could not well measure exactly my elevation above the level of the sea, but I take it not to exceed 40 or 50 toises.

If you think, Sir, this summary worthy the notice of the Royal Society, be so good as to lay it before them.

I am, dear Sir,

Your affectionate humble servant,

Cour de St. Pierre,
Geneve, 13 Avril, 1770.

J. A. Mallet.
Received